Letters

The evidence base for shaken baby syndrome

Response to editorial from 106 doctors

Editor—In challenging the diagnosis of shaken baby syndrome in their recent editorial Geddes and Plunkett make a number of serious errors in interpreting the research on this issue, and they display a worrisome and persistent bias against the diagnosis of child abuse in general.[1]

In their opening sentence Geddes and Plunkett describe shaking a child to "produce whiplash forces that result in subdural and retinal bleeding," omitting the most important element in this condition: brain injury itself. They elaborate that the "theory" of shaken baby syndrome rests on some core assumptions, including that "the injury an infant receives from shaking is invariably severe."

This is in conflict with the research of Alexander et al, Ewing-Cobbs et al, Kemp et al, and Jenny et al, who found that 30%-40% of newly diagnosed shaken baby cases had medical evidence of previously undiagnosed head injury.[2–5] These infants had such mild or non-specific symptoms and signs that their trauma was previously not diagnosed. The diagnosis was ultimately made when the children had subsequent severe episodes of abuse, with computer tomographic evidence of both acute and older subdural haematomata and brain injuries.

Retinal haemorrhages

Geddes and Plunkett then consider retinal haemorrhages. Lantz et al, in the same issue, question the specificity of perimacular folds in abusive head trauma in infancy.[6] They conclude from a literature review that there was no support for the contention that perimacular folds are pathognomonic for abusive head injury. Geddes and Plunkett applied these authors' conclusions not only to perimacular folds but also to retinal haemorrhages.

Although research on the subject of inflicted childhood neurotrauma—over 600 peer reviewed articles—does not claim that retinal haemorrhages are pathognomonic for abuse, it does show that retinal haemorrhages are, overwhelmingly, more common in abuse than in non-inflicted injury. When massive retinal haemorrhages are seen in carefully studied children with non-inflicted major injuries, such as from motor vehicle crashes, crushing head injuries, as in Lantz et al's report, and falls from several storeys, child abuse is not a consideration.

One study analysed these obviously non-inflicted injuries and compared them with abusive head injuries in children under 6 years of age. Severe retinal haemorrhages were seen in 5 of the 233 (2%) children in the non-inflicted group and in 18 of the 54 (33%) in the abuse group.[7] Retinal pathology from *major* trauma mimicking shaken baby syndrome is old news.[8–10] Its incidence is dramatically lower than that resulting from inflicted head injury and because of the obvious major trauma history it does not present a diagnostic dilemma.

Literature on shaken baby syndrome

To discredit the literature on shaken baby syndrome over the past 30 years, Geddes and Plunkett rely on an article by Donohoe.[11] In so doing they have erred in their assessment of the status of the science in the field.

Donohoe's purpose was to examine trends in the quality of scientific evidence. Donohoe used evidence based medicine (EBM) criteria for weighting evidence to judge the comparative merit of published studies published before such criteria were widely embraced by authors, reviewers, and journals. He also plans to apply this process to more recently written articles. He explicitly did not challenge the existence of shaken baby syndrome and, to our knowledge, his review of more recent work has not yet been published. The cited paper reviewed studies published up to six years ago and purposely did not include research that has been published since that time.

One striking limitation of the Donohoe paper is that he used only the keywords "shaken baby syndrome" to search the literature whereas many of the articles on the subject use keywords such as "inflicted childhood neurotrauma," "childhood head injury," "craniocerebral trauma," "inflicted traumatic brain injury," as well as several others. We know of a number of qualified studies that were not included. If the search had been appropriately more inclusive, the resulting conclusions would likely have been quite different.

The application of EBM criteria to judge articles is intended to help physicians discern truth among competing works. The absence of clinical trials and definitive population based studies means lower EBM scores when the work is compared with more definitive work. Low EBM scores, in the absence of more highly regarded work, do not mean that the work is wrong, only that there is room for further research to learn more and that prior conclusions may not be definitive. Many aspects of clinical practice and medical knowledge have not been established with certainty by EBM criteria.

The comparative paucity of well-done population based cohort studies, in the face of a rather large literature of case reports, case series, cohort studies, and case-control studies underscores how hard research in this area is to complete. It also emphasises the need for more research and more government research assistance. Child abuse is a particularly difficult area in which to conduct research. Issues of informed consent, inadequacy of animal models, and the potential legal consequences of participation and telling the truth make this a complicated field.

Short falls in childhood

Geddes and Plunkett claim that "the recent literature contains a number of publications that disprove traditional expert opinion in the field" about short falls in childhood. However, they cite only two publications, and neither disproves the evidence presented in over 25 other studies of short falls in infancy and childhood.

Plunkett cites his own article on fatal falls from short distances in playgrounds, using archived data from various sources.[12] His study has significant problems: the determination of the distances of the falls in the 75 000 cases presented relies on information supplied by the original sources of data and is thus open to question; no infants were studied; several of the falls were from 7 feet (that is, they were not "short" falls); several of the children had crush injuries or pre-existing conditions; and none of the children had "formal retinal evaluation." Nevertheless, Plunkett and others assert that this study "proves" that short falls can kill and cause retinal haemorrhages.

Contact subdural and epidural haemorrhages may, however, result from short falls.[10 w1] They can occasionally cause severe

illness or death from space occupying lesions. Occasionally children with contact injuries due to short falls develop malignant cerebral oedema. Plunkett's fatal cases seem to fall into these categories, as opposed to the whiplash brain injuries associated with immediate concussions seen with severe inflicted head injuries.[w2] Even if one were to accept his conclusions despite these methodological flaws, the study found that death from short falls was still exceedingly rare (18/75 000 = 0.02%). The only other article cited is a review by Ommaya et al that provides no new data and makes sweeping editorial observations unjustified by the literature cited.[w3]

Biomechanical studies

Geddes and Plunkett end by dismissing animal model studies unless they are "validated against the known mechanical properties of the human infant." How are these properties to be known? How can an investigator measure the tensile strength of the *living* infant dura, skull, bridging veins, cerebral cortex, and neck musculature? Although more appropriate studies of the mechanical properties of infant animal brain are beginning to be done,[w4–w7] no current studies reflect the response of infant animal brain tissue to harmonic forces, such as those likely occurring with infant shaking. Although more biologically faithful mechanical models of infants are being constructed,[w8 w9] they will still only approach the response of living infants to shaking.

Asserting that shaking cannot cause infant brain injury, on the basis of current biomechanical studies is premature. Juxtaposed with these mechanical approximations, there is extensive clinical experience and an emerging literature of confessed shaking causing brain injury in infants.[w10]

Conclusion

Child abuse is an enormous social, medical, and mental health problem and its evaluation and treatment have far-reaching implications for children, families, and society. To provide optimal diagnosis and treatment, careful objective research and intellectual honesty are needed and must prevail over the entrenchment of ideological schools of thought and "winning" in court. Unfortunately, there remains considerable difficulty for some doctors to accept that children are abused. We must look at these cases using all of the information available, including collected clinical experience and the synthesis of the best literature on the subject.[w11]

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This letter is signed by another 105 doctors (see bmj.com for details).

Competing interests: None declared.

1 Geddes JF, Plunkett J. The evidence base for shaken baby syndrome. *BMJ* 2004;328:719-20. (27 March.)
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12 Plunkett J. Fatal pediatric head injuries caused by short distance falls. *Am J Forens Med Pathol* 2001;22:1-12.

Details of the other 105 signatories are available on bmj.com, as are details of references w1-w11

Authors' reply

EDITOR—It is difficult to understand how Reece et al could interpret our editorial as displaying "a worrisome and persistent bias against the diagnosis of child abuse in general." Child abuse exists, and we know and attest that it exists. The editorial does not discuss "child abuse in general."

Child abuse exists in many forms: our editorial addresses the diagnostic criteria for a specific type of abuse, the so-called shaken baby syndrome. We emphasise, as have Donohoe and Lantz et al,[1 2] that the literature to support a diagnosis of shaken baby syndrome/inflicted head injury is based on imprecise and ill-defined criteria, biased selection, circular reasoning, inappropriate controls, and conclusions that overstep the data. If it is the questioning of the criteria that is worrisome, we will continue to do so and to cause worry.

We encouraged the readers to evaluate critically the evidentiary basis for a diagnosis of shaken baby syndrome in the light of the questions raised by the two papers. Of course Donohoe's study was limited and would retrieve only papers that included the words "shaken baby syndrome" in the title, key words, or abstract. The lack of scientific rigour that he identified is not restricted to infant head injury papers that specifically mention shaken baby syndrome. If Reece et al perform a critical review of the "number of qualified studies" that they assert would have been included by a wider search, they will encounter the same "data gaps, flaws of logic, and inconsistency of case definition" that were present in the literature studied by Donohoe. We would urge them to look again, for example, at the paper they cite by Alexander et al, where they will find all the above shortcomings.[3]

Finally, we are at a loss to explain or accept the authors' statement in their penultimate sentence: "Unfortunately, there remains considerable difficulty for some doctors to accept that children are abused." If the authors are suggesting that we are among those doctors, or are encouraging others to be so, their argument is a willful misinterpretation of what we have written. When there is new evidence that challenges an established conviction, medicine has the responsibility to critically evaluate the data, and if verifiable, reflect that change. We must have no vested interest in yesterday's belief. We are encouraging doctors to think clearly and critically, even in an area as emotive as child abuse. No more. And no less.

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Competing interests: JFG and JP have given evidence in criminal cases at the request of both the prosecution and the defence.

1 Donohoe M. Evidence-based medicine and shaken baby syndrome. Part I: Literature review, 1966-1998. *Am J Forensic Med Pathol* 2003;24:239-42.
2 Lantz PE, Sinal SH, Stanton CA, Weaver RG Jr. Perimacular retinal folds from childhood head trauma. *BMJ* 2004;328:754-6. (27 March.)
3 Alexander RC, Sato Y, Smith W, Bennett T. Incidence of impact trauma with cranial injuries ascribed to shaking. *Am J Dis Child* 1990;144:724-6.

A full version of this letter is available on bmj.com

Doctors' communication of trust, care, and respect

Details of paper were incorrect

EDITOR—Burkitt Wright et al have not attended one of my group's communication skills courses; yet that doesn't stop them from saying that patients valued forms of communication that are currently not emphasised in training and research, and did not intrinsically value others that are currently thought important, including provision of information and choice.[1] Apart from the breathtakingly absurd suggestion that a qualitative analysis of views of 39 women with breast cancer should overturn painstaking research and survey findings gathered by many, their assertions are factually incorrect.

Firstly, we always ensure that patient needs inform the content of communication skills courses by involving patient groups and considering empirical research findings.

Secondly, patient centredness is a core component of our courses, which includes learning how to tailor information giving, providing choice if wanted, responding appropriately to patient led cues, and expressing empathy and respect.

Thirdly, each day CancerBACUP receives many calls from distressed patients and relatives made anxious and distraught by the lack of information they have received. We need trust, care, and respect, but no convincing evidence exists to show that those things in themselves are enough.

I am indignant that our work and that that of others whom I respect receives such

short shrift from Burkitt Wright et al.[1] The efficacy of our most recent training courses was demonstrated by improving doctors' skills in all the key areas that the authors seem to believe that only they have ever thought about.[2 3] Furthermore, more than 3000 patients in clinics throughout the United Kingdom commented in detail about the communication they received in exit interviews and questionnaires.

Shame on the *BMJ* for publishing a paper that is likely to help undo all the work many of us have been engaged with when trying to produce a sensible framework for communication skills teaching.

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Competing interests: LF's research team have been funded for the past 15 years, developing ways to improve communication skills training for healthcare professionals.

1 Burkitt Wright E, Holcombe C, Salmon P. Doctors' communication of trust, care, and respect in breast cancer: qualitative study. *BMJ* 2004; 328;864-7. (10 April.)
2 Fallowfield LJ, Jenkins VA, Farewell V, Saul J, Duffy A, Eves, R. Efficacy of a Cancer Research UK communications skills training model for oncologists: a randomised controlled trial. *Lancet* 2002;359:650-6.
3 Fallowfield L, Jenkins V, Farewell V, Solis-Trapala I. Enduring impact of communication skills training: results of a 12-month follow up. *Br J Cancer* 2003;89:1445-9.

Paper was muddled

EDITOR—The discussion section of the paper by Burkitt-Wright et al on doctors' communication of trust, care, and respect in breast cancer is extraordinarily muddled.[1] The first paragraph seems to imply that communication style is not of particular concern to patients but then goes on to emphasise the importance of trust, empathy, and autonomy—all of which depend on effective communication.

After asserting that their analysis is more sensitive to patients' needs than everyone else's, the authors make several sweeping and paternalistic generalisations about patients, referring the reader to a set of equally paternalistic, and at times positively misleading, communication rules (boxes 1-3). They even imply that fostering blind faith is more desirable than imparting honest information.

In the sixth paragraph, the most confusing of all, they say that giving patients "the option" (what option?) is better than giving them choice (what on earth do they mean?), but they propose that this has nothing to do with empowerment or shared decision making. They speak approvingly of patients' autonomy but not of determining role preferences, sharing reliable information, discussing more than one option, being open about uncertainties, clarifying values, or any other facets of a mature approach to clinical decision making.

How did this one slip through?

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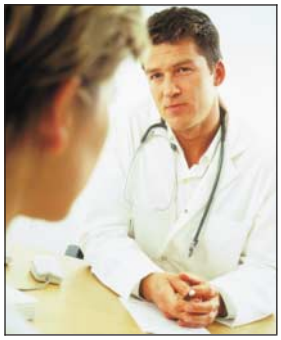
1 Burkitt Wright E, Holcombe C, Salmon P. Doctors' communication of trust, care, and respect in breast cancer: qualitative study. *BMJ* 2004; 328;864-7. (10 April.)

It takes two to make therapeutic relationship work

EDITOR—The qualitative study of Burkitt Wright et al on doctors' communication of trust, care, and respect in breast cancer provides much that is useful for both practitioner and teacher to consider about communicating and functioning well as a healthcare professional.[1] However, any form of relationship by necessity involves more than one person, so the responsibility for this relationship cannot solely rest with the doctor.


BSIP/CABY, VALENCE/SPL

To look at a professional responsibility in isolation ignores the role that the patient has in forming a therapeutic relationship. A follow up study on this group of doctors comparing the doctors' and patients' attitudes about what made a better therapeutic relationship would be interesting. A doctor might act in a particularly useful way in response to the way a patient consults or presents.

The authors also cite evidence that many patients prefer to be directed about treatment rather than given choice, but this leaves the doctor in a dilemma. Patients may claim later that the "direction" they accepted was one they wouldn't have chosen had they received fuller information about alternatives.

Although discovering what patients want out of consultations is crucial, it must be balanced against professional needs. Defensive medicine should not drive the therapeutic relationship, but neither should professional vulnerabilities be ignored.

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1 Burkitt Wright E, Holcombe C, Salmon P. Doctors' communication of trust, care, and respect in breast cancer: qualitative study. *BMJ* 2004; 328;864-7. (10 April.)

Communication needs of all kinds of people should be explored

EDITOR—Smith queries whether the results of the paper by Burkitt Wright et al about doctors' communication with patients with breast cancer are generalisable to other patients.[1 2] Treatment for early breast cancer is unpleasant, but the bulk is likely to be over within a year; the patient then carries on with life as before. Many chronic diseases, however, have a variable course, making diagnosis and prognosis problematic. Patients can face ongoing uncertainty, continuing pain, and increasing, variable impairments. Doctors are experts in medicine or surgery, but if patients rely solely on doctors, they might not learn of other ways of maintaining their lifestyle.[3]

This study excluded patients with acute distress, cognitive impairment, or insufficient English,[1] so the opinions of people in those groups are not known—another reason why the results cannot be generalised. Doctors need to communicate with everyone, and acknowledge that they will need communication support if they lack the skills to communicate with some people directly. People with learning disabilities, for example, face inequalities of access to health care. Coverage for breast and cervical screening are especially poor for women with learning disabilities[4]—obviously relevant to this paper.

Excluding marginalised groups from research perpetuates their marginalisation.[5] Communication research and teaching should be based on "what patients need rather than on what professionals think they need,"[1] but the process must be fully inclusive.

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Competing interests: JC has breast cancer and rheumatoid arthritis.

1 Burkitt Wright E, Holcombe C, Salmon P. Doctors' communication of trust, care, and respect in breast cancer: qualitative study. *BMJ* 2004;328;864-7. (10 April.)
2 Smith R. Editor's choice. The teaching of communication skills may be misguided. *BMJ* 2002;324:0. (10 April.)
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Communication entails more than being nice

EDITOR—At last the medical profession seems to be waking up to the realisation that what is taught under the umbrella of "communication skills" covers too narrow a range of activity.[1] Good communication skills are the output of a number of more sophisticated inputs. Good observation skills, good listening skills, ability to reflect, ability to detect emotional content, and problem formulation are some.

For far too long medical teaching has focused on teaching a narrow set of skills such as "breaking bad news," in the belief that those skills generalise across situations. They do not. The psychology literature has documented this for some time.[2–4] Doctors need a framework of knowledge in which to make a set of skills work. This framework has been notably absent in many undergraduate curriculums, resulting in acquisition of a rather wooden set of "skills" that can be applied only in limited situations.

Patients want medical practitioners who are skilled in the art of sincerity, able to detect emotions, and able to express genuine interest in why they have been consulted. Communication skills is a form of skilled information processing and should be learnt in the way that other skills are learnt, built on a firm knowledge foundation including how information is processed, and practised.

People are happy to accept that doctors don't know everything and even that they make mistakes. What people find harder to swallow is disinterest, clumsy questioning, or ham fisted handling around something that has a lot of meaning for most people—threats to health.

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Competing interests: None declared.

1 Smith R. Editor's choice. The teaching of communication skills may be misguided. *BMJ* 2002;324:0. (10 April.)
2 Jarus T, Gutman T. Effects of cognitive processes and task complexity on acquisition, retention, and transfer of motor skills. *Can J Occup Ther* 2001;68:280-9.
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Authors' reply

EDITOR—Qualitative research is important because it allows patients to express views that may not fit the investigators' or others' preconceptions. We, like some correspondents, were surprised by our findings, so we had to question what we previously thought. Although extensive research and training by some correspondents has greatly improved clinical communication, to imply that we have no more to learn is disturbing. In their different ways, correspondents indicate the need for yet more research to pursue the challenge that these and previous findings present to existing ideas and established interests.[1]

Jones and Willis, Carter, and Bundy all point out the limitations of a "one size fits all" model of communication. Disabled and marginalised people have different needs, while the emphasis on discrete tasks, such as "breaking bad news," misses important functions of communication. Jones and Willis identify patients' role in good communication, and the perils of being too directive. This balance is central to effective communication—and to several commentators' responses. Clinically and intellectually, we find that reconciling patients' need for autonomy with the lead that frightened people seek from their expert clinician is harder than some commentators appear to find it. Coulter thinks that the distinction between respecting patients' "option" and avoiding unwanted choice is muddled, but the muddle reflects the limitations of empowerment of patients, from which perspective she assesses our paper.[2]

In practice, processes of effective communication and communication teaching are rich and complex, as Fallowfield indicates. But practitioners, managers, and researchers respond to the simplifications disseminated in research and government publications. For example, the emphasis on more information means that, locally, patients with breast cancer can receive 10 information leaflets—but some prefer to leave them unread or still feel uninformed.

Clinical communication needs still to be an area of inquiry, not just practice. Research should match the sophistication of what good practitioners already know and do, and it needs to develop new theoretical frameworks within which to evaluate what they do. Claims to patient centredness do little to illuminate or discriminate. We are, with McPherson, looking for more light and less heat.[3]

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Competing interests: None declared.

1 Leydon GM, Boulton M, Moynihan C, Jones A, Mossman J, Boudioni M, McPherson K. Cancer patients' information needs and information seeking behaviour: in depth interview study. *BMJ* 2000;320:909-13.
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Do patients with unexplained physical symptoms pressurise GPs for somatic treatment?

Sorting the wheat from the chaff

EDITOR—The qualitative study by Ring et al asks whether patients with unexplained physical symptoms pressurise their general practitioners for somatic treatment.[1] It seems to be blaming the doctors for not managing the patients properly. As others have pointed out on bmj.com,[2] management is not easy.

I agree that many of these patients may have psychiatric illness and somatisation disorders, but some do not. At what point can medically unexplained symptoms be ignored and not investigated again?

I can think of several "difficult" patients who had extensive investigations over prolonged periods before their slowly developing rare pathology was diagnosed. One was told by two eminent orthopaedic surgeons that the peculiar back pain did not have an organic cause until chronic staphylococcal discitis was diagnosed. Another patient had a retropancreatic abscess diagnosed after several years of complaints of abdominal pain, with normal laboratory and scan results. Retroperitoneal fibrosis, Lyme disease, autoimmune disorders, thyroid disease, diabetes, and many others can present in such an insidious fashion that the diagnosis can be missed for months or even years.

General practice is an art not a science with many patients. You have to keep an open mind or you may miss something treatable.

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Competing interests: None declared.

1 Ring A, Dowrick C, Humphris G, Salmon P. Do patients with unexplained physical symptoms pressurise general practitioners for somatic treatment? A qualitative study. *BMJ* 2004;328:1057-0. (1 May.)
2 Electronic responses. Do patients with unexplained physical symptoms pressurise general practitioners for somatic treatment? bmj.com 2004. http://bmj.bmjjournals.com/cgi/eletters/328/7447/1057 (accessed 21 May 2004.)

Summary of rapid responses

EDITOR—The qualitative study by Ring et al on whether patients with unexplained symptoms pressurise their doctors for treatment together with the accompanying editor's choice provoked a strong response.[1 2] The main criticism levelled at both is that none of the authors is a general practitioner and therefore cannot speak with authority.

Second comes the acknowledgment that there are indeed patients who are difficult and a drain on doctors, although the term "heartsink" is met with scepticism. The idea that hospital consultants might not treat the patients in question any better is also mooted, especially as general practitioners have the edge on other doctors in consultation skills because of their training.

Some correspondents report terrible cases in which a diagnosis was missed and caused unspeakable suffering. Some predict that time and budgetary constraints are a serious obstacle now and will be even more of a hindrance in years to come. Others admit that there are both difficult patients and difficult doctors. Doctors should always be honest and humble, in case they get it wrong, and blaming patients is not the way forward as all known medical conditions were unknown at some point in the past. A number of psychologists recommend that general practitioners do not assume the roles of psychiatrists or psychologists.

Several responses have resource implications. A couple recommend getting a second opinion in doubtful cases but add at the same time that the current culture of British medicine does not favour second opinions. One correspondent recommends delaying any further discussion of unexplained symptoms until the resources are available to order all the tests that are required to make a diagnosis and put doctors' and patients' minds at rest.

All in all, unexplained symptoms seem as puzzling for doctors as they are for patients. A level headed analysis comes from Joan McClusky, a medical writer in New York: general practitioners do the best they can—and so do patients who keep coming back because treatments are not working.

Birte Twisselmann *technical editor*
BMJ

Competing interests: None declared.

1 Electronic responses. Do patients with unexplained physical symptoms pressurise general practitioners for somatic treatment? bmj.com 2004. http://bmj.bmjjournals.com/cgi/eletters/328/7447/1057 (accessed 21 May 2004.)
2 Electronic responses. Insights into intimacy. bmj.com 2004. http://bmj.bmjjournals.com/cgi/eletters/328/7447/0-g (accessed 21 May 2004).

Flaw in WHO Framework Convention on Tobacco Control

Letter identified wrong problem with the framework convention

Editor—Sehmi writes that the WHO Framework Convention on Tobacco Control has a major flaw.[1] However, the main problem with the way the framework convention handles smokeless tobacco is not that it is inadequate but that it is undifferentiated from smoking tobacco. However, in many crucial respects—notably, risk of death and disease—smokeless and smoking tobacco are very different, with much lower risk arising from the obvious physical difference of drawing volatile gaseous and particulate products of combustion into the lungs compared with chewing or sucking the dried and cured leaf.

The framework convention does not handle "harm reduction" at all well—but it may be a very effective health policy to allow markets in smokeless tobacco to develop to enable smokers to stop smoking while continuing to use nicotine in tobacco form. Evidence from Sweden shows the high prevalence of oral snuff (snus) use among men is highly likely to be the reason for Sweden's lowest rate of smoking in the developed world and, as a result, the lowest rates of cancer.[2]

Policies that bear down on smokeless tobacco, such as attempts to ban it, may have adverse consequences if they cause more people to use cigarettes, or if they remove a much less hazardous option for continuing nicotine use from smokers who are unwilling or unable to stop. The European Union leads the world in this folly by banning products such as snus outside Sweden, despite the evidence that it has a strong public health benefit.[3]

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Competing interests: None declared.

1 Sehmi KS. WHO Framework Convention on Tobacco Control has major flaw. *BMJ* 2004;328:958. (17 April.)
2 Foulds J, Ramstrom L, Burke M, Fagerström K. The effect of smokeless tobacco (snus) on smoking and public health in Sweden. *Tobacco Control* 2003;12:349-59.
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Question is scientific rather than cosmetic

Editor—The final text of the Framework Convention on Tobacco Control as agreed at the World Health Assembly in May 2003 has the following in Article 1 under "The use of terms": "Tobacco products' means products entirely or partly made of the leaf of tobacco as raw material to be used for smoking, sucking, chewing or snuffing."[1]

As such it covers the question raised by Sehmi in his letter,[2] which suggests the need for an amendment to the convention. The research on snus seems to point to a reduced risk, and thus raises a scientific rather than a cosmetic question. In several countries of the Pacific, tobacco from cigarettes is widely used, along with betel quid. This is also another form of chewing; although cosmetic appeal is certainly absent, its harm seems not to be as damaging as inhaled tobacco smoke.

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Competing interests: None declared.

1 World Health Organization. Tobacco free initiative (TFI). Available at: www.who.int/tobacco/areas/framework/final_text/en/index1.html (accessed 20 May 2004).
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Who should decide on caesarean sections?

Editor—Given the argument against medicalisation of natural processes such as birth and NICE's recent guidance on caesarean section on demand,[1] the increasing rate of caesarean deliveries points at its justified use in the name of safety and personal choice (otherwise termed as elective).

In the developing world, however, especially in India, elective caesarean section is yet to become popular and leaves the choice open for obstetric surgeons. In this context, private health care institutions may encourage caesarean deliveries for the prolonged postpartum stay in the hospital. In other words, the profit motive seems stronger than its justified use for maternal safety. My study with Ramanathan found that caesarean delivery is four or more times more likely in private facilities than in the public sector health facilities in India.[2] In addition, evaluations of the indications for caesarean delivery often show that the reason cited on records is fetal distress. In such circumstances, should caesarean deliveries be allowed to replace vaginal deliveries and be the necessary tool of safe motherhood in this century?

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1 Kmietowicz Z. NICE advises against caesarean section on demand. *BMJ* 2004;328:1031. (1 May.)
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BMJ papers could include honesty box for research warts

Editor—The *BMJ* includes a small box with each paper, summarising the prior knowledge and what the study adds. This is laudable, but inevitably overlaps with the abstract. We suggest an alternative use for a box. We are torn between calling it an honesty box or a confessional box.

The idea is that all research has warts, some ugly, others less so. The ugly ones should be picked up by peer review. The less ugly ones are never seen, remaining only as a twinge of guilt in the researcher's conscience.

Possible examples are:

- "Our power calculation—though justified by the literature—was optimistic"
- "Reference 13 covers similar ground to our study, and we did not know it was in progress when we planned ours"
- "We didn't expect finding B, and did the literature search on it after it was discovered."

Declaration of competing interests does not serve the purpose. We believe that an honesty or a confessional box is in the spirit of genuine scientific inquiry, and it may act as an antidote to spin. It may even help to restore public faith in science.

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Confessional/honesty box

This letter may improve our CVs